

experiences when propelling the ship under natural conditions, it follows that the same setting of the shutters which suits a given engine when working with its highest speed and power will also approximately suit it when eased down to its lowest.

It remains to be explained in detail how it is proposed to carry out the operation in dealing with any given ship.

The ship, before she leaves the dock for the trial of her machinery, will have the instrument mounted as described, in place of her screw. The casing will be provided with proper apertures, capable of being closed at will, to permit the egress of air and the ingress of water as the dock fills. The casing will thus be in a condition to receive the moment of rotation delivered by the screw and communicate it to the recording apparatus.

The arrangement of the dynamometric apparatus presents no difficulty. In this, the downward pull delivered by the lever operates vertically on the middle of a flat horizontal steel spring, which is supported at both ends; and it is proposed so to proportion the spring that its maximum deflection shall be about $1\frac{1}{2}$ inches. Different springs, however, would be required for engines of widely different power.

Reference has previously been made to the amount of heat developed by friction in the friction brake, as probably the most formidable of the objections to its employment when the horsepower to be dealt with is as large as that now contemplated. But it must not be supposed that the absorption of the same amount of work in the instrument that has been described will fail to be converted into the same amount of heat here also. The dynamic theory of heat is unquestionable as a theory, and the quantitative relation of work and heat is known with certainty within far narrower limits than deserve even to be mentioned in reference to the present subject. Although, however, the extinction of say 2,000 horse-power will in fact here, as well as in the friction brake, consist in its conversion into so many units of heat, the circumstances of the conversion are entirely different in the two cases, and the difference is such as to obliterate here the inconvenience which was fatally great there. There, the heat was to be dealt with as being constantly developed between surfaces in close contact and inaccessible to water. Here, it will be making its appearance in the body of a mass of water; and though the rapidity of the development will be so great that the whole contents of the casing would be quickly raised to boiling heat if the heat had no escape, yet, in the first place, there is a considerable refrigerating power always at work, since the whole casing is enveloped in cold water, and, moreover, there is no difficulty in creating a constant change of water within the casing sufficient to keep down the mean internal temperature to any limit which may be thought proper. For instance, when the instrument is dealing with 2,000 horse-power, the temperature would be kept well below the boiling point if in each minute eight cubic feet of cold water be substituted for the same quantity of the hot contents of the casing, nor would the exactness of the dynamometric action be in the smallest degree impaired by the substitution.

Mr. Froude in his valuable paper, to which we are glad to call attention, thus summarises briefly the advantages which would be derived from the system of submitting marine engines to dynamometric trial. It is certain that a very large but unmeasured amount of power is wasted, in friction and otherwise, between the cylinders and the propeller; and that the amount probably differs, both in respect of difference in type of engine and in respect of goodness of construction and workmanship. The chief difficulties which thus arise are as follows:—

- (1) The speed attained by a given ship, driven by a given indicated horse-power, fails to measure discriminatively the merits of the ship.
- (2) No means exist of testing which type of engine delivers the largest proportion of the power which it indicates.
- (3) No test exists by which to measure concisely the specific constructional merit of this or that engine, or to determine the relative constructional merit of the engines supplied by different firms.

The dynamometric test would remove at once each of these difficulties, by substituting a final and real test for a collateral and to a large extent a delusive one. For to rely exclusively on the test furnished by the indicator is almost equivalent to testing the power of a horse solely by the quantity of food he consumes and digests, or the efficiency of a boiler solely by the quantity of coal per hour it will legitimately consume on its firebars.

Table of Reference Letters used in Diagrams and Drawings.

- A. Screw shaft.
- B. Turbine.
- C. Casing.
- D. Diaphragms.
- E. Sliding regulating shutters.
- F. Screws for moving E, governed by telescopic rods actuating bevel gear controlled from ship's deck.
- G. Lever for holding casing.
- H. Links connecting G with dynamometric apparatus.
- K. Knife-edged gimbal for carrying strain of H to spring.
- L. Framed radius for guiding K and eliminating oblique strains.
- M. Dynamometer spring.
- N. Suspension links carrying the ends of M.
- O. Feeler conveying elastic motion of M.
- P. Telescopic rod taking rotation of screw shaft by bevel gear and communicating it to integrating apparatus.
- Q. Motion axis of integrating apparatus governed by O.
- R. Automatic integrator.
- S. Bell crank for magnifying motion of O and conveying it to paper cylinder.
- T. Paper cylinder recording magnified motion of O.
The graphic integration of the record given by T is comparable with the automatic integration given by R.
- U. Shed covering integrating apparatus.
- V. Strong balk brackets upholding U.

THE COMMISSION OF THE FRENCH ACADEMY AND THE PASTEUR-BASTIAN EXPERIMENTS

I further reply to a communication of mine to the Academy of Sciences of Paris on July 10, 1876, and as his latest contribution to a controversy which grew out of it, M. Pasteur, at the *séance* of January 29, 1877, threw down a very definite challenge.

The discussion was raised according to M. Pasteur by my statement, "that a solution of boiled potash caused bacteria to appear in sterile urine at 50° C., after it had been added to the latter in quantity sufficient for exact neutralisation," and he then said:—"I defy Dr. Bastian to obtain, in the presence of competent judges, the result to which I have referred with sterile urine, on the sole condition that the solution of potash which he employs be pure, *i.e.*, made with pure water and pure potash, both free from organic matter. If Dr. Bastian wishes to use a solution of impure potash I freely authorise him to take any in the English or any other Pharmacopœia, being diluted or concentrated, on the sole condition that that solution shall be raised *beforehand* to 110° for twenty minutes, or to 130° for five minutes. . . . This is clear enough, it seems to me, and Dr. Bastian will understand me this time."

At the *séance* of February 12 my reply was read. The essential part of it was as follows:—"During the last week I have repeated my experiments several times, and with a degree of precaution going much beyond the severity of the conditions prescribed by M. Pasteur. . . . I repeated them at first with liquor potassæ which had been previously raised to 110° C. for sixty minutes, and afterwards with liquor potassæ which had been raised, in the same manner, to 110° C. for twenty hours. The results have been altogether similar to those produced upon sterile urine by liquor potassæ which has been raised only to 100°, when added in suitable quantity; that is to say, in twenty-four to forty-eight hours the urine was in full fermentation and swarmed with bacteria."

After the reading of this reply, M. Pasteur asked the Academy to appoint a Commission to report upon the subject in dispute, and at the next meeting of the Academy (February 19) it was announced that "MM. Dumas, Milne Edwards, Boussingault sont désignés pour constituer la Commission qui sera appelée à exprimer une opinion sur le fait qui est en discussion entre M. le Dr. Bastian et M. Pasteur."

The following correspondence then ensued:—

20, Queen Anne Street, W., February 27, 1877

DEAR SIR,—I was pleased to learn, from the *Comptes Rendus*, yesterday, that the Academy had appointed you together with MM. Milne Edwards and Boussingault to act as a Commission to "express an opinion on the fact" now under discussion between M. Pasteur and myself.

I can scarcely suppose that the Commission would deem it expedient to express an opinion on this subject without having an opportunity of seeing both M. Pasteur and myself perform our respective experiments.

I write, therefore, to inform you that if a convenient time can be arranged, I shall be very happy to come to Paris for three days

in order to perform my experiments before the Commission which has been nominated by the Academy.

I should, moreover, feel much obliged if you will have the goodness to inform me exactly what steps the Commission proposes to take, and how the precise terms for formulating the question of fact which is to be submitted to their consideration are to be settled. It appears to me that these terms ought, in the first place, to be agreed upon between M. Pasteur and myself.

Faithfully yours,

H. CHARLTON BASTIAN

Monsieur Dumas, le Secrétaire perpétuel,
Académie des Sciences

No reply to this letter was received, though a translation of it was published shortly afterwards in the *Comptes Rendus*. The first letter which subsequently came to hand on this subject was the following:—

Académie des Sciences, Paris, le 5 mai, 1877

MONSIEUR,—Je crains que la lettre que j'ai eu l'honneur de vous adresser il y a trois semaines ne vous soit pas parvenue, et je prends la liberté de vous faire savoir de nouveau que la Commission chargée par l'Académie des Sciences de prendre connaissance de vos expériences est prête à vous recevoir. Elle a déjà demandé à M. Pasteur d'opérer sous ses yeux.

Puisque vous avez accepté de venir à Paris, tout est préparé pour vous recevoir et dès votre arrivée, si vous voulez bien m'en informer, le laboratoire de l'Ecole Normale, ou tout autre, seront mis à votre disposition.

Agréez, Monsieur, l'assurance des mes sentiments les plus distingués,

J. B. DUMAS

Paris, rue St. Dominique, 69

20, Queen Anne Street, W., London, May 8, 1877

DEAR SIR,—On February 27 I had the honour of informing you that I was willing to come to Paris to perform some experiments before the Commission appointed by the Academy, if a convenient time could be arranged. I asked also to be informed as to the steps the Commission proposed to take, and how the precise question submitted to them for report was to be agreed upon.

I anxiously awaited a reply to this letter for some time, but none came.

This morning I had the honour of receiving a letter from you, bearing the date of May 5, which was reposted to me from a wrong address, viz., 81, Avenue Road, Regent's Park. Therein you state that you had written to me three weeks previously. I shall be glad if you will be good enough to inform me to what address this first letter was sent, as it has not yet come to hand; and I find, on inquiry, that it has not been delivered at 81, Avenue Road, where I resided two years ago. On receipt of this information I will make further inquiries at the General Post Office.

The letters which I have had the honour of addressing to you concerning my communications to the Academy have always borne the address which stands at the head of this sheet.

Three weeks ago, if the arrangements made by the Commission had been satisfactory, I could have gone to Paris without much inconvenience; now, however, my engagements, both public and private, will not permit me to leave London, and I fear it may be impossible for me to go to Paris till about the third week in July, when our medical session will terminate.

Meanwhile I trust to be able to recover your first letter, and I hope to be fully informed, not only as to the precise question on which the Commission is to report, but as to the mode in which the Commission will conduct the inquiry. I am still anxious, in fact, to receive that information for which I asked in my letter of February 27.

Believe me, dear Sir, faithfully yours,

Monsieur Dumas

H. CHARLTON BASTIAN

18 mai, 1877, Paris

MONSIEUR,—Je me suis empressé de vous faire un duplicata de la lettre que j'avais eu l'honneur de vous adresser au nom de la Commission de l'Académie des Sciences, dès qu'elle avait été délivrée des soins de la séance publique tenue le 23 avril, et qui ne vous était pas parvenue.

J'ai vu M. Pasteur. Il se tient à votre disposition pour le 15 juillet, époque à laquelle vous seriez libre de venir à Paris.

M. Lockyer, qui a passé quelque jours ici, s'est chargé de vous dire combien nous désirons voir avec vous vos expériences et avec quelle entière liberté d'esprit elles seront appréciées.

Agréez, Monsieur, l'assurance de ma considération la plus distinguée,
J. B. DUMAS

rue St. Dominique, 69

Académie des Sciences, Paris, le 25 avril, 1877

Duplicata.—Le Secrétaire perpétuel de l'Académie à Monsieur le Docteur Charlton Bastian, 20, Queen Anne Street, à Londres

MONSIEUR,—La Commission nommée par l'Académie des Sciences pour examiner le dissentiment qui s'est élevé entre M. Pasteur et vous a consacré plusieurs séances à suivre les expériences de M. Pasteur. Elle est donc en mesure de s'occuper des vôtres.

Puisque vous avez offert de venir les répéter devant elle à Paris, elle se met à votre disposition, et elle vous offre le laboratoire qu'il vous plaira de désigner pour les accomplir. Vous choisirez vous-même, après les avoir visités, celui qui vous conviendra le mieux. M. Pasteur vous prie de considérer le sien comme tout à vos ordres.

La Commission, avant d'engager tout examen de la question, a pensé qu'il convenait d'abord de voir les expériences mêmes, réalisées en liberté par leurs auteurs. S'il y a lieu d'ouvrir plus tard entre elles une comparaison contradictoire, elle en déterminera les conditions, en vue de donner, à son opinion, une base certaine.

Le premier élément de l'enquête à laquelle vous avez souscrit, M. Pasteur et vous, devait consister, en effet, à donner à chacun de vous l'occasion de produire les faits sur lesquels vos opinions respectives se fondent.

Agréez, Monsieur, l'assurance de mes sentiments les plus distingués.

J. B. DUMAS

20, Queen Anne Street, W., May 24, 1877

DEAR SIR,—I have the honour to acknowledge the receipt of the duplicate of the missing letter, bearing date April 25, and also your note of May 18, the assurances in which were very gratifying to me.

Your official letter of April 25 contains some information in regard to the conduct of the inquiry by the Commission, which I have been for some time desirous of obtaining. In respect to these proposed proceedings I may perhaps now be permitted to make some observations, in order, as far as possible, to avoid the chance of any misunderstanding between M. Pasteur and myself and the Commission, during the progress of the inquiry.

I am anxious, in fact, to define (1) what I understand to be the object of the Commission, and (2) to explain to what extent I am prepared to submit to its judgment. I desire to do this in order that I may have the honour of learning from you whether I am correct in this understanding, and whether my submission to the extent to be specified is all that the Commission will expect from me.

1. I gather from the *Comptes Rendus* of February 19, that the Commission has been appointed that it may "express an opinion upon the fact" under discussion between M. Pasteur and myself; and the fact in question seems to me to be this:—*Whether previously boiled urine, protected from contamination, can or cannot be made to ferment and swarm with certain organisms by the addition of some quantity of liquor potassæ which has been heated to 110° C., for twenty minutes at least.* M. Pasteur asserts that he has not seen fermentation occur under these conditions, whilst I assert that I have; so that the point of principal importance would seem to be to ascertain whether such positive results can be reproduced before the Commission. I learn, therefore, with much satisfaction, that the Commission will allow to each of us the opportunity of reproducing before it the facts upon which we found our respective opinions. This, indeed, I regard as an essential condition of the inquiry.

2. If the Commission proposes to limit itself to reporting upon this mere question of fact I will freely submit to its decision. If, however, it does not propose thus to restrict itself, and is empowered to express an opinion upon the interpretation of the fact attested, and on its bearings upon the "Germ Theory of Fermentation," or "Spontaneous Generation," then I must respectfully decline to take part in this wider inquiry.

I feel compelled to adopt this decided position because my stay in Paris must be limited to three or four days; and if any other questions beyond that above specified were subsequently raised by the Commission demanding the performance of some new experiments, either by M. Pasteur or myself or by both of us, the inquiry, instead of being limited to a few days, might be prolonged indefinitely.

I desire, therefore, to obtain the assurance of the Commission that no new experiments shall be demanded from either of us, except with the full concurrence of both M. Pasteur and myself. Under these circumstances I will undertake, so far as it lies in my power, to be in Paris by the 14th of next July, in order to place myself at the disposal of the Commission.

Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

20, Queen Anne Street, W., June 21, 1877.

DEAR SIR,—One month ago (May 24) I had the honour of writing to you to ask for some official information as to the precise scope of the inquiry to be made by the Commission appointed by the Academy of Sciences, before whom I have been invited to appear. To this letter I have as yet received no reply, so that I do not even know whether it has been received.

I have made arrangements which will enable me to go to Paris and perform my experiments before the Commission at the time named in your letter of May 18, namely, about July 15, but naturally before taking part in any arbitration I desire to receive some official intimation as to the exact terms and scope of the question which has been submitted to the arbitrators. I know not whether the few lines which I saw in the *Comptes Rendus* of February 19 announcing the nomination of the Commission, contain also the only instructions which have been given to it, or whether any other and fuller instructions exist. No information has been communicated to me and I am, unfortunately, not acquainted with the custom of the Academy in regard to commissions of this kind.

Craving the favour of an early reply,
Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

MONSIEUR,—Il est parfaitement entendu que la Commission de l'Académie des Sciences sera le 15 juillet à votre disposition.

Il est également qu'elle desire, si c'est possible, n'avoir à s'occuper que de l'expérience de M. Pasteur et de la vôtre, au sujet de l'urine traitée par la potasse.

Vous n'avez donc aucun motif de craindre qu'elle ait besoin de vous demander un séjour prolongé.

Veillez agréer, Monsieur, l'assurance de ma considération la plus distinguée.
J. B. DUMAS

r. St. Dominique, 69

20, Queen Anne Street, W., July 6, 1877

DEAR SIR,—I beg to acknowledge the receipt of a letter from you which came to hand on June 29.

I do not find in it any distinct acceptance of the conditions mentioned in my letter of May 24, as those upon which alone I should be prepared to repeat my experiments before the Commission, viz., (1) the limitation of the report to the question of fact mentioned, (2) the assurance that no new experiments shall be demanded from either of us except with the full concurrence of both M. Pasteur and myself.

I might infer from your silence that no objection is raised to these restrictions, but before leaving for Paris I must receive your definite assurance that this is so.

Not being thoroughly proficient in the French language I presume the Commission will permit me to avail myself of the services of some French friend as an interpreter. I also trust that the Commission will provide for the taking of shorthand notes of any discussion during the progress of the investigation of which the Commission, M. Pasteur, or myself may desire to have a record.

On the receipt of a favourable reply you may expect me to be in Paris on Saturday morning, the 14th inst., otherwise I shall be most reluctantly compelled to decline to participate in the inquiry.

Believe me, dear Sir, faithfully yours,
Monsieur Dumas H. CHARLTON BASTIAN

Paris, 12 juillet, 1877

MONSIEUR,—La Commission de l'Académie des Sciences sera dès le 15 à votre disposition.

Elle est prête à vous entendre ; mais elle desire, comme vous, que son examen soit borné au point en discussion entre vous et M. Pasteur. Ce serait seulement au cas où vous desireriez aller plus loin qu'elle aurait à examiner si le temps lui permet d'entreprendre davantage, votre séjour étant très court.

M. Edwards, membre de la Commission, parle très bien l'anglais.

Dès votre arrivée vous auriez la bonté de m'en informer, rue St. Dominique, 69.

Agréez, Monsieur, l'assurance de ma considération la plus distinguée.
J. B. DUMAS

Having received this acceptance of the limitations which I had specified, I left London for Paris on July 13.

On the afternoon of July 15, I met the Commission by arrangement at the laboratory of M. Pasteur, at the Ecole Normale Supérieure. The Commission was represented by MM. Dumas and Milne Edwards, M. Boussingault having been compelled to withdraw on account of a recent domestic affliction.

The first stage of our discussion was the announcement to me by M. Milne Edwards of his objection to the second condition mentioned in my letter of July 6, and of his determination to take no part in the inquiry if I still adhered to this condition. M. Dumas' letter of July 12, in the name of the Commission, and on the faith of which I had come to Paris, was thus at once set aside.

M. Milne Edwards contended that he could not take part in any Academy Commission which had not full power to vary the experiments at discretion ; whilst I, on the other hand, contended that my stay in Paris must, as I had said from the first, be limited to a few days, and that I could not see my way, therefore, to consent to the initiation of new experimental conditions. I further urged that the Commission had been appointed to report upon a simple question of fact, that M. Pasteur had challenged me to obtain certain results, before "competent judges," that I had come to Paris to repeat certain well-defined experiments before them, and that they were commissioned to express an opinion thereon and on the experiments of M. Pasteur to the Academy of Sciences.

A very long discussion ensued, but no satisfactory conclusion was arrived at. In the evening I wrote the following note to M. Dumas :—

Grand Hôtel St. James, Paris, July 15, 1877

DEAR MONSIEUR DUMAS,—After our conference this afternoon I had a long conversation with M. Pasteur, and am going to his laboratory early to-morrow morning, to show him the mode in which I make my experiments. I shall thus be enabled to learn what precise alterations he would desire in order that the experiments may be conducted in a manner satisfactory to himself.

Afterwards I trust it may be more possible for me to meet the wishes of the Commission in regard to the inquiry, and I hope you will therefore be able to make it convenient to see me for a few minutes at your own house to-morrow at 1.30 P.M.

If you are able to do this, pray do not take the trouble to answer this note. Should it not be convenient to you, perhaps you will kindly send a few words to me to the care of Professor Würtz, upon whom I am to call about noon.

Believe me, dear Sir, faithfully yours,
À Monsieur Dumas H. CHARLTON BASTIAN

At my interview with M. Dumas on Monday, July 16, I proposed a kind of compromise. The proposition was that on the present occasion we should have "the first element" of the inquiry as defined by M. Dumas in his letter of April 25 ; viz., that the opportunity should be given to M. Pasteur and myself of repeating (without variation) the actual experiments upon which we based our respective opinions ; that I should then return to London, and after the Commission had expressed its opinion to M. Pasteur and to myself as to any variations in the experimental conditions which they might desire to institute, that I should return to Paris to witness and to perform such modified experiments.

The names of MM. Fremy, Trécul, Robin, and Würtz had been mentioned as persons one or other of whom I should like to see placed on the Commission in succession to M. Boussingault. But at the meeting of the Academy that afternoon it was announced that M. van Tieghem had been nominated to succeed M. Boussingault. This gentleman being a former pupil and present colleague of M. Pasteur, the Commission was left without a single member who could be considered as representing my views, or even as holding a neutral position between me and my scientific opponent.

The next day I received the following note from M. van Tieghem :—

Paris, 17 juillet, 1877

MONSIEUR LE DOCTEUR,—La Commission de l'Académie se réunira demain, mercredi, à huit heures du matin au laboratoire de M. Pasteur à l'Ecole Normale. Je viens, en son nom, vous prier

de vouloir bien vous y trouver pour procéder à la mise en train des expériences en litige.

Veuillez agréer, Monsieur, l'expression de mes sentiments les plus distingués.

PH. VAN TIEGHEM,
Membre de la Commission

I made all the necessary arrangements that afternoon in M. Pasteur's laboratory for the performance of my experiments, and the next morning at eight o'clock M. Pasteur and I were at the appointed place. M. van Tieghem was also there, and shortly afterwards M. Milne Edwards arrived. He apparently had had no communication with M. Dumas since the time of my interview, and when told, in reply to a question of his, of the proposition which I had made to M. Dumas, M. Milne Edwards very hastily expressed his disapproval of it, and at once, without listening further, left the laboratory. He was followed by M. van Tieghem. I remained, and after one hour M. van Tieghem returned. He informed me that, having waited in vain for the arrival of M. Dumas, M. Milne Edwards had at length gone away.

I remained in conversation with M. van Tieghem for nearly an hour in an upper room of M. Pasteur's laboratory. When we came down, much to my surprise, we learned from M. Pasteur that M. Dumas had arrived, that he had been told of the departure of M. Milne Edwards, and that he also had then left, saying that the Commission was at an end—but without in any way communicating either with his colleague, M. van Tieghem, or with myself.

Thus began and ended the proceedings of this remarkable Commission of the French Academy.

July 30

H. CHARLTON BASTIAN

NOTES

FROM correspondence which we have received, we gather, that because we omitted to state in our leading article of last week the fact that London is the only University which treats science as a necessary branch of education, that article has been thought hostile to the University of London. The fact in question is of course well known and appreciated, but it did not seem to us to be relevant. Our article had reference to the question of Universities as against Examining Boards rather than to the quality of the examinations. We heartily acknowledge the good the London Examining Board has done, and the obligations under which it has placed science and scientific men.

THE Annual Conference of the Royal Archaeological Institute of Great Britain and Ireland commences, on the 7th proximo, at Hereford, for a week. The Bishop of Hereford is president.

AN important resolution of the International Geodetic Congress is now being carried out. The Montsouris observatory is being connected by telegraphic observations with Bonn and Berlin in Germany, and with Geneva and Neufchatel in Switzerland. Two astronomers from Berlin having arrived in Paris, and M. Loewy, member of the French Academy of Sciences, with two assistants, having arrived in Berlin from Paris, the work has been at once proceeded with. The wires are freed a few hours every night for obtaining comparisons. The connection with Geneva and Neufchatel is executed, *via* Lyons, by Commander Perrier, of the staff, and the operations have been continued to Marseilles and Algiers. The comparison between the Montsouris and Paris observatories will be a work of triangulation, the two establishments being about a gun-shot from each other.

A NUMBER of Abyssinians have arrived in Paris on their way to London. They are encamped in the Acclimatisation Gardens (Bois de Boulogne), with camels, elephants, ostriches, &c., and other animals destined to the London Zoological Gardens. The heads and manners of the blacks have been scientifically examined by Dr. Broca, and a report on them will be read at the French Society for the Advancement of Science at Havre.

THE Bureau of the French Association to meet in Havre on the 23rd instant, consists of Prof. Broca, president; M. Kuhl-

mann, vice-president; M. Deherain, general secretary; M. Perrier, vice-secretary; M. Masson, treasurer. Most of the French railway companies give half-price tickets to persons going to the Association. The hotel proprietors in Havre guarantee a certain number of beds; furnished apartments have also been largely promised, and the berths in one of the Transatlantic Company's steamers have been placed gratuitously at the disposal of members.

AN interesting account of the recent falling of a mountain in Tarentaise, Savoy, causing disaster to two flourishing villages, has been communicated to the *Courrier des Alpes*, by M. Bérard. The phenomenon has been incorrectly reported as instantaneous, and the destructive effect complete, whereas the case is that of a mountain which for twenty days, without cessation, has been dismembering itself and literally falling night and day, into the valley below, filling it with piled-up blocks of stone, extinguishing all sounds by its incessant thunder, and covering the distant horizon with a thick cloud of yellowish dust. The entire mass comprised in the slope forms a mutilated cone 200 metres broad at the top and 600 at the base (the slope being about 50°); this is composed of blocks of hard schist lying close together, but no longer united; and it is united to the body of the mountain only by a vertical mass 40 to 50 m. thick, which already is fissured and shaken. Periods of repose occur lasting only a few seconds or a minute at most; then the movement recommences, and continues about 500 hours. Blocks of 40 cubic metres become displaced with no apparent cause, traverse the 1800 m. of descent in thirty seconds, leaping 400 or 500 m. at a time, and finally get dashed to pieces in the bed of the torrent, or launch their shattered fragments into the opposite forest, mowing down gigantic pines as if they were so many thistles. One such block was seen to strike a fine fir-tree before reaching the bridge between the villages; the tree was not simply broken or overthrown, but was crushed to dust (*volatilisé*); trunk and branches disappeared in the air like a burning match. Rocks are hurled together and broken into fragments that are thrown across the valley like swallows in a whirlwind; then follow showers of smaller fragments, and one hears the whistling sound of thousands of pebbles as they pass. M. Bérard reached the edge of the rock (2,460 m. high), on one of the sides of the falling cone, and ventured along it, obtaining a good view of the "terrifying" spectacle. He reaffirms his conviction that the phenomenon is inexplicable by any of the usual reasons that account for Alpine disturbances, such as penetration of water, or melting of snows, or inferior strata in motion; nor does the declivity of the slope explain it. His hypothesis is that some geological force is at work, of which the complex resultant acts obliquely to the axis of the mountain and almost parallel to its sides.

ACCORDING to M. Perrin, an eighth or a tenth portion of the French army is incapable of doing good service, in consequence of indistinct vision. M. Perrin formally proposes to remedy this by the adoption of spectacles. It is affirmed that spectacles are useful, if not indispensable, to 47 per cent. of the officers coming from the École Polytechnique.

FROM the Annual Report of the Council of the Royal Society of New South Wales, we gather that the membership at the beginning of the session of 1877 was 298, and that the Society is in a generally flourishing state. A considerable access of activity has occurred since the establishment of sections (nine) last year. The Council are hopeful of obtaining an annual endowment from the Government.

FOR want of space the gigantic Giffard captive balloon will not be constructed, as was anticipated, in the Paris Exhibition, but special ground will be granted as we announced a few months ago. The